Analyzing simulation experiments with common random numbers (Part I, 2nd rev. and expanded version)

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COMMON RANDOM NUMBERS

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Abstract

To analyze simulation runs which use the same random numbers, the blocking concept is not needed. Instead this paper applies a linear regression model with a non-diagonal covariance matrix. This covariance matrix does not need to have a specific pattern such as constant covariances. Further, this paper proposes a new framework for the error analysis. This framework consists of three factors (common random numbers, replication, model validity), each with three levels. A simple example yields surprising results.

Keywords

Blocking, variance reduction, estimated generalized least squares, general linear model, error analysis

1. Introduction

A popular variance reduction technique in simulation is the use of the same random numbers. Not only academic researchers have advocated common random numbers, but also practitioners apply this technique. Actually it is the only variance reduction technique, practitioners find simple and intuitively appealing. The technique is simple indeed, since all it takes - see Table 1 - is to reset the random number seed to its old initial value before executing the next run with different values

TABLE 1

Data of Simulation Experiment

Factor combination (effects $\beta_1 \cdots \beta_Q$)	Replicated responses (seed 1)	(seed 2)	...	(seed m)	Average response	Estimated (co)variances
$x_{11} \cdots x_{1Q}$	y_{11}	y_{12}	...	y_{1m}	$\bar{y}_1$	$\hat{\sigma}_1^2 \; \hat{\sigma}_{12} \cdots \hat{\sigma}_{1n}$
$x_{21} \cdots x_{2Q}$	y_{21}	y_{22}	...	y_{2m}	$\bar{y}_2$	$\hat{\sigma}_2^2 \cdots \hat{\sigma}_{2n}$
$x_{i1} \cdots x_{iQ}$	y_{i1}	y_{i2}	...	y_{im}	$\bar{y}_i$	$\hat{\sigma}_i^2 \cdot\cdot \hat{\sigma}_{in}$
$x_{n1} \cdots x_{nQ}$	y_{n1}	y_{n2}	...	y_{nm}	$\bar{y}_n$	$\hat{\sigma}_n^2$

for the simulation variables x (in a queuing simulation x_1 may denote the number of servers and x_2 the server speed). We run n combinations of simulation variables or "factors", where $n > Q$. Common random numbers imply that the n responses within one column of Table 1 use the same seed. As we shall see a good statistical analysis requires that the experiment be repeated (with different seeds), that is, the number of replications m should satisfy the condition: $m_i > 2$ $(i = 1,\ldots,n)$. We assume that all replications use common seeds; hence m_i is a constant, m. We shall discuss the right-hand part of Table 1 in the next section.

We note that in steady-state simulations the m replicates can be interpreted in different ways. For example, we make m (long) runs each starting in some fixed state (such as the empty state in queuing simulations) and using m different seeds. (Interpretations are more difficult in renewal analysis, and similar more sophisticated analyses.) In terminating simulations (and most practical simulations are terminating) the interpretation is straightforward. Kleijnen (1986) discusses steady-state versus terminating simulations, renewal analysis, and so on.

A problem is that common random numbers complicate the statistical analysis of the simulation data. (Since practitioners tend to neglect that analysis, they may not be aware of any complication.) We assume that the goal of the experiment with the simulation model is to estimate the effects (say) β of Q independent variables x (the simulation model has k input variables, with $k > 1$, which correspond with $Q > k+1$ independent variables in the regression model; see sections 3 and 4). An efficient and effective solution of this estimation problem, is a factorial design, for example, a 2^{k-p} design with integer p satisfying $0 < p < k-1$; see Kleijnen (1986).

In the literature on factorial designs blocking is a classical concept. Originally blocking was introduced to reduce heterogeneous, uncontrollable influences in experiments with real (non-simulated) systems [see John (1980), Lorenzen (1984), Peres (1981), Shoukri and Ward (1984), Steinberg and Hunter (1984, pp. 85-86)]. Later on, some authors interpreted common random numbers in simulation experiments as block effects [see Anderson and Sargent (1974, p. 134), Lin and Rardin (1979, p. 1261-1262), Naylor et al (1967, p. 324), Schatzoff (1981, pp. 853-854), Schruben (1979, pp. 239, 247-248)]. Other authors, however, doubted this interpretation [Kleijnen (1974/1975, p. 355), Nozari et al. (1984), Wilson (1984)]. This paper shows that we do not need the blocking model; instead we analyze the simulation data through a linear regression model with a non-diagonal covariance matrix. Compared to the blocking model our model is more general, and yet very simple (see Section 2). Further we propose a new framework, consisting of three dimensions or factors (namely, random number seeds, replication, and model validity), each factor with three levels (Section 3). We add an example, so simple that it provides surprising results (Section 4).

We hope to stimulate a new (and maybe final) discussion on the fundamental and practical problem of analyzing simulation experiments with common random numbers.

2. Common random numbers and least squares

To analyze simulation experiments with common random numbers, we shall propose a linear regression model (see eq. 2.2) with a statistical submodel for the regression residuals; that submodel reflects the use of common random numbers. The responses within the same column of Table 1 use the same seed, and hence they are dependent. If we repeat each factor combination i a fixed number of times ($m_i = m > 2$), unbiased estimators of $\sigma_{ii'} = \mathrm{cov}(y_{ir}, y_{i'r})$ are

$$\hat{\sigma}_{ii'} = \frac{\sum_{r=1}^{m} (y_{ir}-\bar{y}_i)(y_{i'r}-\bar{y}_{i'})}{m-1} \qquad (i,\ i' = 1,\dots,m) \tag{2.1}$$

Responses in different columns of Table 1 are independent, because they use independent seeds: y_{ir} and $y_{i'r'}$ are independent if $r \neq r'$. So there are m independent observations on the n-variate vector $\underset{\sim}{y} = (y_1,\dots,y_n)'$. The n×n elements $\hat{\sigma}_{ii'}$ of eq. (2.1) define the estimator $\hat{\underset{\sim}{\Omega}}_y = (\hat{\sigma}_{ii'})$. Obviously common seeds imply that $\underset{\sim}{\Omega}_y$ and hence $\hat{\underset{\sim}{\Omega}}_y$ are no longer diagonal (we hope that all $\sigma_{ii'}$ are positive so that common seeds indeed work as a variance reduction technique). We emphasize that we do not assume a specific pattern for the covariance matrix, i.e., we do not assume constant covariances; constant covariances are assumed in Schruben and Margolin (1978), Schruben (1979), Nozari et al. (1984), Safizadeh (1983).

We assume that the relationship between the expected value of the simulation output $E(y) = E(\bar{y}) = \mu$ (Table 1 shows m vectors y and one vector $\bar{y} = (\bar{y}_1,\ldots,\bar{y}_n)'$, each vector with n elements) and the simulation input X (an n×Q matrix) is linear in the parameters β (a vector with Q effects β_j). The actual simulation output deviates from the expected output; we assume that these disturbances $e = (e_1,\ldots,e_n)'$ are additive:

$$y = X\beta + e \tag{2.2}$$

The blocking model is also linear, but it decomposes the additive error into a "blocking" component and a "remaining" error; for exact definitions and interpretations in a simulation context we refer to the proponents of that model, for example, Schruben and Margolin (1978, pp. 512-513).

We propose two different point estimators of the effects β, namely the Ordinary Least Squares (OLS) estimator

$$\hat{\beta} = (X'X)^{-1}X'\bar{y} \tag{2.3}$$

and the Estimated Generalized Least Squares (EGLS) estimator

$$\hat{\tilde{\beta}} = (X'\hat{\Omega}^{-1}X)^{-1}X'\hat{\Omega}^{-1}\bar{y} \tag{2.4}$$

Simple (but tedious) linear algebra proves that it is indeed correct to replace the individual responses y_{ir} by the averaged responses $\bar{y}_i$, assuming a constant number of replications ($m_i = m$).

Note: If the number of replications were not constant ($m_i \neq m$), then OLS gives more weight to factor combinations which are replicated more often. This weighting can be achieved, replacing $\bar{y}$ in eq. (2.3) by the vector with the nm individual responses $(y_{11}, y_{12}, \ldots, y_{n(m-1)}, y_{nm})'$ and $\underset{\sim}{X}$ by an nm×Q matrix with first m_1 identical rows $(x_{11}, \ldots, x_{1Q}) \cdots$, finally m_n rows $(x_{n1}, \ldots, x_{nQ})$. The EGLS estimator is given by eq. (2.4) which uses the averages $\bar{\underset{\sim}{y}}$, provided we replace $\hat{\underset{\sim}{\Omega}}_y$ by $\hat{\underset{\sim}{\Omega}}_{\bar{y}} = (\hat{\sigma}_{ii'}/m_i)$. Also see Arnold (1981), Schmidt (1976).

It is possible that the random matrix $\hat{\underset{\sim}{\Omega}}_y$ has no inverse. Indeed we experienced nearly singular $\hat{\underset{\sim}{\Omega}}_y$ when using common seeds. To solve this problem we may add replications, assuming that the population covariance matrix $\underset{\sim}{\Omega}_y$ is not singular. Given the m replications, we may also estimate $\underset{\sim}{\Omega}_y$ after deleting one or more replications (or columns in Table 1). Deleting observations leads to jackknifing, evaluated in Kleijnen et al. (1986). We can manipulate not only the estimator $\hat{\underset{\sim}{\Omega}}_y$ but also the population matrix $\underset{\sim}{\Omega}_y$, that is, if we delete one or more factor combinations (rows in Table 1) then $\underset{\sim}{\Omega}_y$ changes into a smaller matrix. Later on, we can use the deleted observations to validate the regression model; see Kleijnen (1983, 1986).

Besides the point estimators we need variance estimators (standard errors). Obviously we have for OLS:

$$\hat{\underset{\sim}{\Omega}}_{\hat{\beta}} = (\underset{\sim}{X}'\underset{\sim}{X})^{-1}\underset{\sim}{X}'\hat{\underset{\sim}{\Omega}}_y\underset{\sim}{X}(\underset{\sim}{X}'\underset{\sim}{X})^{-1}/m \tag{2.5a}$$

Following an idea in Schruben and Margolin (1978, pp. 515-516) we can easily prove that eq. (2.5a) is equivalent to:

$$\widehat{\mathrm{cov}}(\hat{\beta}_j, \hat{\beta}_{j'}) = \frac{\sum_{r=1}^{m} (\hat{\beta}_{jr} - \hat{\beta}_j)(\hat{\beta}_{j'r} - \hat{\beta}_{j'})}{(m-1)m} \qquad (j, j' = 1, \ldots, Q) \tag{2.5b}$$

where $\hat{\beta}_{jr}$ denotes the estimator of effect β_j computed from replication r:

$$\hat{\underset{\sim}{\beta}}_r = (\underset{\sim}{X}'\underset{\sim}{X})^{-1}\underset{\sim}{X}'\underset{\sim}{y}_r \tag{2.6}$$

where $\underset{\sim}{y}_r$ denotes the rth observation on $\underset{\sim}{y}$; $\sum_r \hat{\beta}_{jr}/m$ is identical to $\hat{\beta}_j$ of eq. (2.3). Eq. (2.5b) is analogous to eq. (2.1) and will be used in section 4. For GLS (with known Ω_y) we have

$$\underset{\sim}{\Omega}_{\tilde{\beta}} = (\underset{\sim}{X}'\underset{\sim}{\Omega}_y^{-1}\underset{\sim}{X})^{-1}/m \tag{2.7}$$

For EGLS we replace $\underset{\sim}{\Omega}_y$ in eq. (2.7) by its estimator $\hat{\underset{\sim}{\Omega}}_y$ and the resulting $\hat{\underset{\sim}{\Omega}}_{\hat{\tilde{\beta}}}$ holds asymptotically; see Schmidt (1976). Kleijnen, Cremers, Van Belle (1985) 's Monte Carlo experiment suggests that the asymptotic covariance matrix applies if $m_i > 25$. However, their result should be used with care, since they studied EGLS without common seeds ($\sigma_{ii'} = 0$ if $i \neq i'$) albeit with heterogeneous variances ($\sigma_i^2 \neq \sigma^2$).

We emphasize that the analyst should not use the standard formula $\underset{\sim}{\Omega}_{\hat{\beta}} = (\underset{\sim}{X}'\underset{\sim}{X})^{-1}\sigma^2$ since that formula holds only if the errors are independent with common variance ($\underset{\sim}{\Omega}_y = \sigma^2 I$). The estimated covariance matrices $\hat{\underset{\sim}{\Omega}}_{\hat{\beta}}$ and $\hat{\underset{\sim}{\Omega}}_{\hat{\tilde{\beta}}}$ are used to apply the Student t test, assuming the responses y (or errors e) are <u>normally</u> distributed:

$$t_\upsilon = \frac{\hat{\beta}_j - \beta_j}{\hat{\sigma}_{\hat{\beta}}} \qquad (j = 1,\ldots,Q) \tag{2.8}$$

where $\hat{\sigma}_{\hat{\beta}}$ is the square root of the jth element on the main diagonal of $\hat{\underset{\sim}{\Omega}}_{\hat{\beta}}$ and υ denotes the degrees of freedom of the t statistic. Kleijnen et al. (1985) suggest to take $\upsilon = \min(m_i - 1) = m-1$. For EGLS we replace $\hat{\beta}_j$ by $\hat{\tilde{\beta}}_j$ in eq. (2.8). Because $\hat{\underset{\sim}{\Omega}}_{\hat{\tilde{\beta}}}$ holds only asymptotically, we may replace t_υ by the standard normal variable (say) z, assuming $m > 25$. The sensitivity of the t statistic to nonnormality, and alternative regression analyses (such as rank regression and jackknifing) are discussed in Kleijnen (1986) and Kleijnen et al. (1986).

Which estimation procedure should we use, <u>OLS or EGLS</u>? If the covariance matrix $\underset{\sim}{\Omega}_y$ were known, then the GLS estimator $\tilde{\beta}$ would be the Best Linear Unbiased Estimator (BLUE). Actually we have to estimate the covariance matrix $\underset{\sim}{\Omega}_y$. We saw that we do not know the exact properties of the resulting OLS and EGLS estimators. A general rule in science is to try different models, when analyzing a problem. Here we recommend to apply both OLS (eq. 2.3 combined with eq. 2.5) and EGLS (eqs. 2.4 and 2.7 provided $m > 25$?) to the same simulation data, and to see if the two techniques give the same qualitative conclusions. In one case-study (the Rotterdam container harbor) the two techniques did give similar conclusions: both OLS and EGLS identified the same factor as being important while all other factors were non-significant; see Kleijnen, Van den Burg and Van der Ham (1979). If OLS and EGLS give qualitatively different conclusions, then we may add factor combinations to the n old combinations. For example, if the factors are quantitative, we may simulate that combination where the two models give predictors furthest apart, within the area of interest (interpolation, no extrapolation); next we select the model with predictor which is closest to the simulated response $\bar{y}_{n+1}$; see Kleijnen (1986) for a further discussion on "model discrimination".

Note: The Generalized Linear Model of eq. (2.2) is indeed useful in the interpretation of simulation data, including validation, sensitivity analysis, and optimization. Kleijnen (1986) gives many references to applications of regression analysis as metamodels of simulation models.

3. A new framework

We propose a novel framework for the "error" analysis of simulation data, i.e., the analysis of the error component $\underset{\sim}{e}$ in the linear model of eq. (2.2); in section 4 we shall demonstrate that framework. We now utilize the concepts of experimental design itself, i.e., we distinguish three factors, each with three levels. The three factors are:

1. Random number seed.
2. Replication.
3. Validity of the regression (meta)model.

For factor 1, seed, we distinguish the levels a, b and c:

(a) We use the same random number seeds (per replication of the n combinations of the k factors of the simulation model; these k factors correspond to Q independent regression variables, where $Q > k+1$; see section 1).

(b) We sample all seeds independently ($n \times m$ independent responses).

(c) We synthesize (a) and (b), i.e., assuming that the simulation model has multiple inputs ($k > 1$), we sample some seeds (namely $k_1 \geq 1$) independently and some seeds ($k_2 \geq 1$) we keep constant ($k = k_1 + k_2$) per replication; see Mihram (1972, 1983), Chang et al. (1985), Wilson (1984).

For factor 2, <u>replication</u>, we also distinguish three levels:

(a) There are no repetitions ($m_i = 1$ with $i = 1,...,n$).

(b) There are repetitions ($m_i \geq 2$ for all i).

(c) There are pseudoreplications, i.e., we assume that the simulation model has a steady state (and satisfies additional technical assumptions) so that we can estimate the variances σ_i^2 from single runs (we can use subruns, renewal analysis, spectral analysis, standardized time series, etc; see Kleijnen, 1986).

For factor 3, <u>metamodel validity</u>, we again distinguish three levels:

(a) The regression model, used to analyze the simulation data, is correct. Then we estimate the variance from the estimated residuals ($\hat{e} = y - \hat{y}$), provided the variance is constant ($\sigma_i^2 = \sigma^2$) and we have degrees of freedom available ($n > Q$).

(b) The regression model is not valid. For example, the metamodel ignores higher-order effects (such as quadratic effects); see level (c).

(c) The regression model is approximately valid. For example, the regression model is a first-order polynomial whereas the simulation model should be approximated by a second-order polynomial; however, within the area of interest (local approximation!) the neglected second-order effects (curvature and interactions) may be small compared to the first-order effects and the noise σ_i^2; also see the example in next section.

We have not yet worked out this concise framework (of three factors with three levels) in full detail, but we do illustrate its use in the following section. We hope that our new framework will be used by other researchers too.

4. An illustration

We illustrate the above framework through the simplest example we can imagine in the context of this paper. So we assume that the true simulation model is

$$y_{ir} = \beta_0 + \beta_1 x_i + e_{ir} \text{ with } e_{ir} \sim \text{NID}(0,\sigma^2)$$
$$(i = 1,\ldots,n) \quad (r = 1,\ldots,m_i) \tag{4.1}$$

Consequently, if we specify the regression model correctly, then the simulation model and the regression model have the same structure (no specification error). We examine three factor values: n = 3. If we use the same seeds and a single replicate ($m_i = 1$), then we obtain identical errors ($e_{11} = e_{21} = e_{31}$); see Figure 1. Because the error variances are constant ($\sigma_i^2 = \sigma^2$) in eq. (4.1), we use OLS point estimators $\tilde{\beta}$ (we shall discuss EGLS in eqs. 4.2 through 4.5). Obviously, in this example we obtain a perfect estimate of the slope and an imperfect estimate of the intercept: $\hat{\beta}_{11} = \beta_1$ and $\hat{\beta}_{01} = \beta_0 + e_{.1}$ where $e_{11} = e_{21} = e_{31} = e_{.1} = \sum_i e_{i1}/n$.

If we obtain repetitions ($m_i = m > 2$) and we use common seeds (see Figure 1: $e_{12} = e_{22} = e_{32} = e_{.2}$), then this simple example still yields perfect OLS point estimates of the slope in each replication ($\hat{\beta}_{1r} = \beta_1$) and imperfect estimates of the intercept ($\hat{\beta}_{0r} = \beta_0 + e_{.r}$). Consequently, repetition yields a perfect estimate of the variability of the slope estimator [$\widehat{\text{var}}(\hat{\beta}_1) = \text{var}(\hat{\beta}_1) = 0$] and a valid estimator of the variability of the intercept estimator [$\widehat{\text{var}}(\hat{\beta}_0)$]; also see eq. (2.5b).

If we have no repetitions ($m_i = 1$), then we cannot estimate the variability of the estimators $\hat{\beta}_0$ and $\hat{\beta}_1$, unless we assume a constant variance ($\sigma_i^2 = \sigma^2$). In the example we specified the regression model correctly, and we obtain estimated residuals all equal to zero ($\hat{e}_{i1}=0$). Hence we correctly conclude that $\text{var}(\hat{\beta}_1) = 0$ and incorrectly we conclude that $\text{var}(\hat{\beta}_0) = 0$.

Figure 2 illustrates a <u>misspecified</u> regression model, i.e., the regression model is still (incorrectly) specified by eq. (4.1) whereas the true simulation model now equals eq. (4.1) augmented with the second-order term $\beta_2 x_i^2$. How can we detect this specification error, and what are the consequences if we do not detect the lack of fit? If we have repetitions ($m > 2$) then two tests are possible.

(i) We compare the estimated residuals $\bar{y}_i - \hat{y}_i$ to the "pure error" $\hat{\sigma}_i^2$ defined in eq. (2.1). This comparison leads to the "F test for lack of fit" popular in experimental design, assuming constant variances ($\sigma_i^2 = \sigma^2$) and normality; see Kleijnen (1974/75, 1986).

(ii) We use the estimated effects $\hat{\beta}$ to derive the predictor $\hat{y}$ ($= \hat{\beta}_0 + \hat{\beta}_1 x$) for a new factor combination ($\underset{\sim}{x}_{n+1} \neq \underset{\sim}{x}_i$) and compare this $\hat{y}$ to the observed simulation response y_{n+1}. This validation test leads to a t test. This test is simplest if we make $\hat{y}_{n+1}$ (depending on y_i) and y_{n+1} independent, i.e., if we use a new seed for y_{n+1}. Also see Kleijnen (1983, 1986).

We may not detect the specification error, especially if the second-order effect β_2 is small compared to the first-order effect β_1 and the noise σ_i^2 (we tend to reject $H_0 : \beta_1 = 0$ and accept $H_0 : \beta_2 = 0$). If we have repetitions, then we estimate σ^2 using $\hat{\sigma}_i^2$ of eq. (2.1); the estimators $\hat{\sigma}_i^2$ remain unbiased, even if we use the wrong regression model ("pure error estimators" known from the Analysis of Variance in experimental design). If we have single replicates ($m_i = 1$), then we saw above that we must assume constant variances ($\sigma_i^2 = \sigma^2$). Unfortunately, in case of specification error the estimated residuals ($\hat{e}_{i1} = \hat{y}_{i1} - y_{i1}$) do not provide an unbiased estimator of σ^2. We note that in Figure 2 not all estimated residuals $\hat{e}_{i1}$ equal zero (whereas in Figure 1 we had $\hat{e}_{i1} = 0$ so that we correctly concluded that $var(\hat{\beta}_1) = 0$ and incorrectly we concluded that $var(\hat{\beta}_0) = 0$).

We now return to the simplest case, namely both the simulation and the regression model are specified by eq. (4.1). What happens if we use EGLS instead of OLS? Common seeds imply that all n input combinations have the same error per replication:

$$e_{ir} = e_{.r} = \frac{\sum_{i'=1}^{n} e_{i'r}}{n} \qquad (r = 1,\ldots,m) \tag{4.2}$$

Eq. (4.1) yields

$$\bar{y}_i = \frac{\sum_{r=1}^{m} y_{ir}}{m} = \beta_0 + \beta_1 x_i + e_{i.} \qquad (i = 1,\ldots,n) \tag{4.3}$$

where $e_{i.} = \sum_{r=1}^{m} e_{ir}/m$. However, common seeds imply that in this example all input combinations have the same error (say) $e_{..}$, that is, eq. (4.2) yields

$$e_{i.} = \frac{\sum_{r=1}^{m} e_{ir}}{m} = \frac{\sum_{r=1}^{m} e_{.r}}{m} = e_{..} \qquad (i = 1,\ldots,n) \tag{4.4}$$

Substitution of eqs. (4.1) through (4.4) into eq. (2.1) yields

$$\hat{\sigma}_{ii'} = \frac{\sum_{r=1}^{m} (e_{ir}-e_{i.})(e_{i'r}-e_{i'.})}{m-1} = \frac{\sum_{r=1}^{m} (e_{.r}-e_{..})^2}{m-1} \tag{4.5}$$

so that $\hat{\sigma}_{ii'}$ reduces to a constant, say $\hat{\sigma}^2$. Consequently the estimator $\hat{\underset{\sim}{\Omega}}_y$ has all n×n elements equal to a common constant (and the estimated correlation coefficients $\hat{\rho}_{ii'} = \hat{\sigma}_{ii'}/\hat{\sigma}_i\hat{\sigma}_{i'}$ are all equal to one: maximum linear correlation). Hence $\hat{\underset{\sim}{\Omega}}_y$ is singular and EGLS is not possible. So the estimates clearly warn the researcher not to apply EGLS in the example of eq. (4.1) where all variances and covariances are constant. (In realistic examples the errors e_{ir} will have heterogeneous variances σ_i^2 and non-constant correlations $\rho_{ii'}$ so that EGLS does apply).

We emphasize that to analyze the example of eq. (4.1) with common seeds, we do not need the blocking model.

5. Conclusion

In the statistical analysis of a simulation experiment we must specify a statistical model. Our model provides an alternative to the blocking model. The ideal model should be both realistic and simple. Our model is indeed more realistic, since we do not assume a specific covariance pattern. Our model is also quite simple, as we can use the well-known OLS point estimators combined with the corrected standard errors (see eqs. 2.3 and 2.5) and EGLS (see eqs. 2.4 and 2.7 assuming we have "many" replications, say $m > 25$). We also proposed a new framework that may be useful in future research on simulation output analysis.

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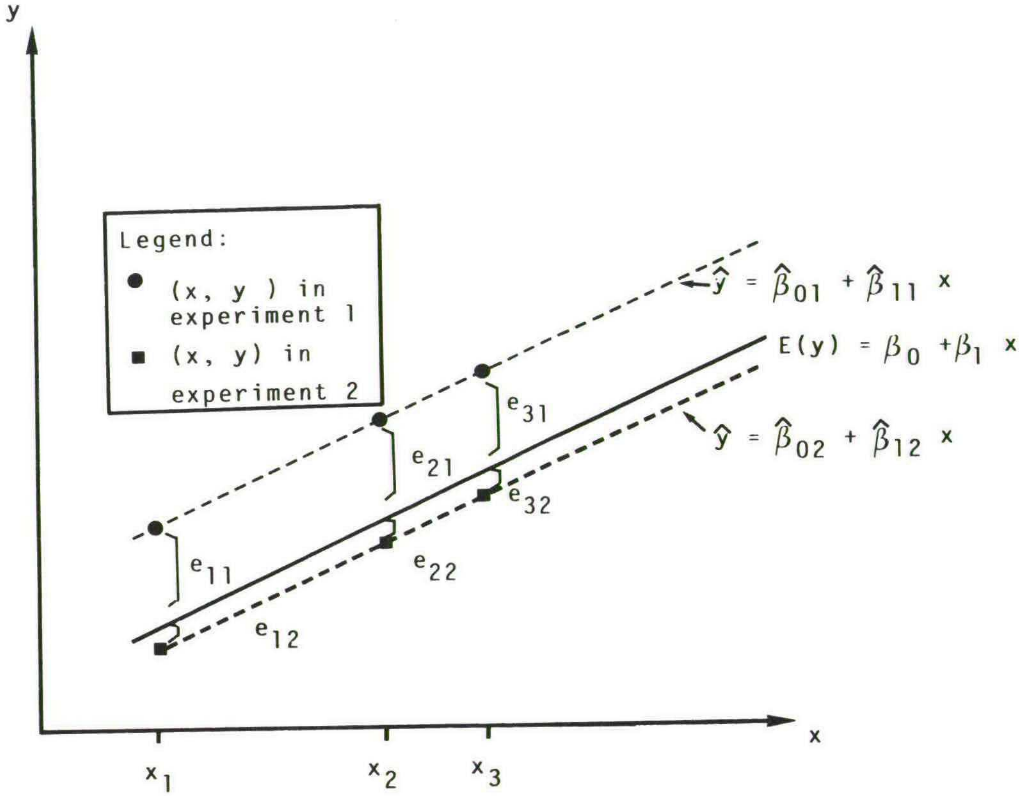

FIGURE 1. Sampling from $y_{ir} = \beta_0 + \beta_i x_i + e_{ir}$ with common random numbers ($i = 1,2,3$) ($r = 1,2$).

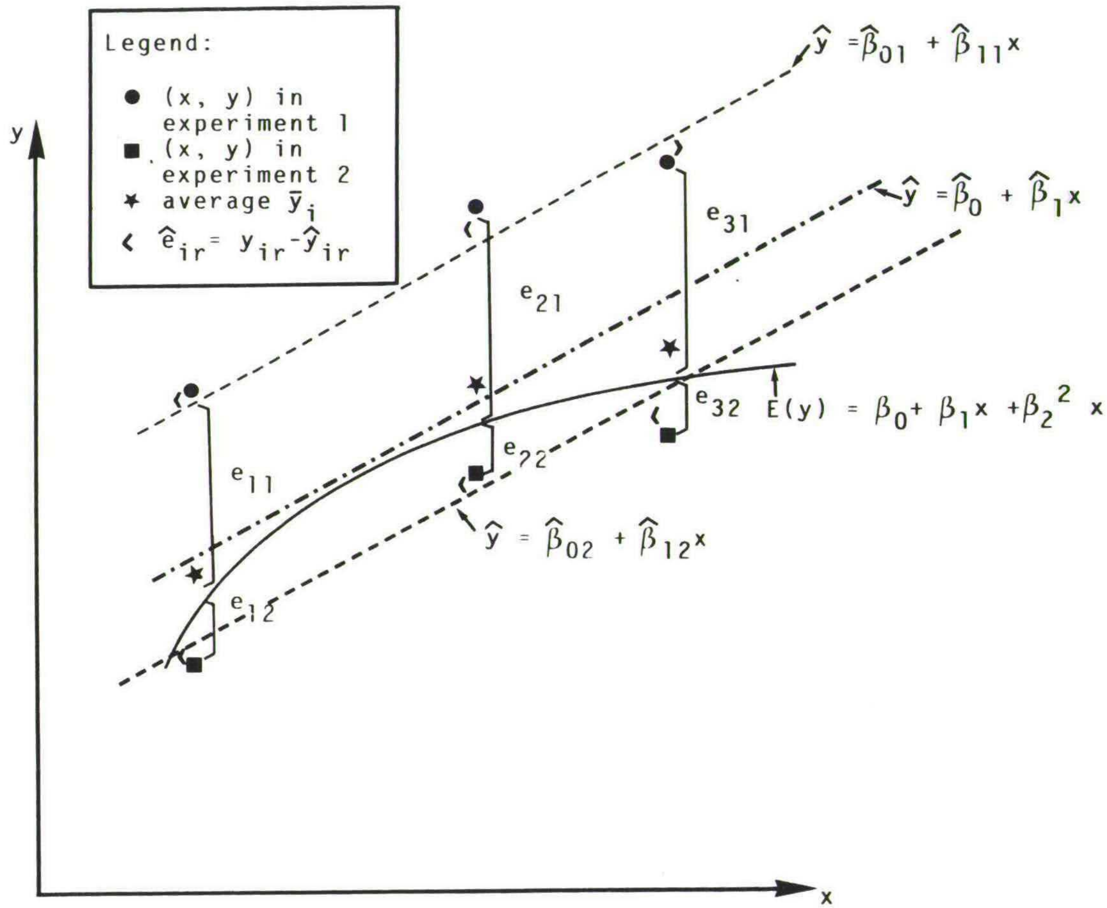

FIGURE 2. Sampling from $y_{ir} = \beta_o + \beta_1 x_i + \beta_2 x_i{}^2 + e_{ir}$ with common random numbers, while estimating $\hat{y}_i = \hat{\beta}_o + \hat{\beta}_1 x_i$.

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